On seeing it in print, however, a natural answer occurs to me, which it may be worth while to give. The whole point of the reasoning depends on assumed properties of vacuum.

The assumptions are as follow :—

(1) That a perfect vacuum is an absolute non-conductor of electricity.

(2) That no contact E M.F. exists between a metal and a vacuum.

(3) That vacuum has a specific inductive capacity.

Grant all these, and the argument is sound. Decline to admit any of them, and it proves nothing. Break down the first two of them, and it proves too much: it proves the non-existence of any thermal contact-force whatever between conductors. For if there were any E.M.F. at the metallic contact, and none at the other or vacuum contacts, a continuous current would flow, propelled by energy derived from a cold place.

This argument is indeed the ordinary one to prove that the algebraic sum of the E.M.F.'s at all the junctions of a closed conducting circuit in which no energy but heat is supplied must be zero when the temperature is uniform.

The proof scarcely holds when insulators are interposed, though the *fact* may be true nevertheless. When chemically active substances with their extraneous supply of energy are interposed, the fact itself is no longer true. But how do we know what is true when vacuum is interposed? The hypothesis on which the argument is founded is a baseless conjecture.

But it may be said, Are not the hypotheses probable? Do you not yourself believe them? I believe in (1) and (3) provisionally, but certainly not in (2). The contact E.M.F. between two substances is probably some surface action or skin phenomenon, and I see no reason why it should not occur as well in the boundary between metal and void as in the boundary between one metal and another. Indeed, it is not improbable that the sum of the E.M.F.'s in *every* circuit of chemically inert substances, whether conducting or not, and inclusive of vacuum, is zero under uniform temperature conditions.

All that is wanted to establish this is the knowledge that in a circuit of any one substance at non-uniform temperature the total E.M.F. shall be zero,[1] or that the Thomson effects in a single substance always balance each other; *i.e.* that the total E.M.F. in a circuit shall depend on a potential function of temperature, or $dE = f'(t)dt$.

Now it is quite true that this $f'(t)$ is the Peltier coefficient divided by absolute temperature, and that $f(t)$ in its most general form contains an arbitrary constant, but what of that? Nothing is known of $f(t)$ except that it is a potential function: it is not known to represent any physical effect. I never said that the Peltier effect enabled us to find the most general form of the function $f(t)$; I said it gave us the E.M.F. at a junction.

And there is much ground for the assertion; for it is easy to show that in a simple AB circuit, with junctions at t_1 and t_2, the total E.M.F. is

$$E = \Pi_1 - \Pi_2 + \int_{t_2}^{t_1} (\Theta_A - \Theta_B)dt\,;$$

just as if the resultant E.M.F. were the algebraic sum of two Peltier E.M.F.'s and of two Thomson E.M.F.'s.

My only contention is that this equation, which is undeniably true when the Π are interpreted as heat-coefficients, is also true and immediately interpretable when they stand for contact E.M.F.'s. The burden of proof as to the physical existence of an unnecessary and in every sense arbitrary constant rests with those who doubt this simple explanation.

It is difficult to see how a doubt can arise, or how the Peltier and Thomson productions or destructions of heat can be accounted for without local E.M.F.'s. Nohow, so Dr. Hopkinson has proved, and I also have insisted (*Phil. Mag.*, October 1885, and March 1886), except by some wildly gratuitous assumption of an actual physical specific heat for electricity, dependent on the temperature and on the metal in which it happens to be.

Liverpool, December 14, 1889. OLIVER J. LODGE.

Mirages.

THE article in NATURE of November 21, 1889 (p. 69), recalls to me mirages I saw in March 1888, while travelling in the East on the steam yacht *Ceylon*.

On the 29th we were crossing the Black Sea from Sebastopol. It was a fine cool day and quite calm. In the afternoon a false or mirage horizon about 3° above the true one was visible for a few hours. No objects were within range of vision. The mirage disappeared as the sun declined.

The next day was very much warmer, and we saw a more marked and interesting mirage in the afternoon as we were steaming across the Sea of Marmora away from Constantinople. In this case it appeared only in the west, and objects were seen reflected in an inverted position. A small conical-shaped island was seen with its inverted image at times distinct from and at times blending with the original. The image was distinctly seen of some land, which was actually below the horizon. The mirage of the reflection of the sun in the sea was, when seen through a glass, especially beautiful. It resembled a glorious cataract of golden water. This mirage lasted till quite the dusk of the evening, and then gradually thinned down and died away.

I do not know whether mirages at sea are uncommon; but as the officers on board did not remember seeing one before, I thought these instances might be worth recording.

ARTHUR E. BROWN.

Thought Cot, Brentwood, December 31, 1889.

Self-luminous Clouds.

I AM very sorry that I took no notes, some six or seven years ago, on the first and only occasion of my seeing self-luminous clouds, but though I can give neither date nor positions, the following facts are still fresh in my memory.

Passing through Bushey Park after dark, I noticed an aurora borealis, and, as I had only recently seen the rather rare phenomena of the rays of the setting sun converging towards a point in the east, I followed the direction of one of the principal beams of light towards the south, when, at a point somewhat south of my zenith, I noticed an equatorial belt of luminous clouds. I found that each cloud belonged to a ray, and faded and brightened with it, but was separated by about 60° of clear sky. This belt of clouds extended down to the western horizon, the eastern one was obstructed by trees, while shortly afterwards small dark clouds appeared on that side, and the sky soon became overcast.

The luminous clouds were quite transparent, so that even faint stars could be seen through them when at their brightest. I have heard from Scandinavian captains that these luminous belts are sometimes seen in northern latitudes, and are sure signs of bad weather. I have written these few remarks in the hope that those of your readers who may have the chance of seeing an aurora borealis will also look out for these clouds, and if possible determine their position. C. E. STROMEYER.

Strawberry Hill, January 4.

The Revised Terminology in Cryptogamic Botany.

THE anglicized forms of most of the terms in common use, employed in the "Hand-book of Cryptogamic Botany" recently issued by Mr. G. Murray and myself, have not up to the present time found much support from our fellow-botanists. I propose, therefore, to give, in some detail, the reasons which have induced us to adopt them, and to urge their general use on writers on cryptogamic botany. For this purpose we will take as our text extracts from three reviews of the "Hand-book," marked, as all the critiques have been, with only one or two exceptions, by a generous appreciation of the difficulties of our task, and a too great leniency to the many shortcomings of the work :—"The most conspicuous, though not the most important, of these [changes] is the adoption of anglicized terminations for Latin and Greek technical words. This is a matter in which it is hard to draw the line aright. . . . As a matter of taste we think the authors have gone much too far in this direction. They complain of the 'awkwardness and uncouth form of these words'; we should have thought the reproach applied much more strongly to 'cœnobe,' 'sclerote,' 'nemathece,' and 'columel'" (NATURE). "An Englishman may guess what 'archegone' is short for, for example; but why puzzle a foreigner with a new form of a word with which he is familiar in every treatise hitherto written on the special subject in any European language?" (*Academy*). "Too sanguine expectations on this head might well be toned down by remembering the complete failure of the somewhat similar experiment made by Lindley. . . . Primworts, spurgeworts, bean-capers, and hippurids are decidedly simpler, even if less euphonious, than Primulaceæ, Euphorbiaceæ, Zygophyllaceæ, and Halorageæ; yet the longer Latin terms are still universally used, while the quasi-English ones have never obtained even temporary acceptance" (*Journal of Botany*).

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[1] Hopkinson virtually pointed this out, *Phil. Mag.*, October 1885.

The last of these criticisms appears to rest on a confusion between the principles of nomenclature and those of terminology. In nomenclature, rigid rules have been laid down, and accepted by all leading naturalists of all countries, in order that the scientific names of species, genera, orders, &c., may correspond in scientific treatises in all languages. In the terminology of flowering plants no such rule has ever been attempted to be laid down; but each writer, when writing in his own language, uses terms, usually of classical origin, and derived from common roots, but of a form as far as possible amenable to the laws of the language in which he writes. All that we are contending for is the extension of the same principle to cryptogamic botany; one of the main objects in the publication of our "Hand-book" being to make the study of flowerless plants as attractive to the public at large as is that of flowering plants.

In order to show how recent is the universal adoption of this practice in phanerogamic botany—a change largely due to the influence of Dr. Lindley's writings—we append a list of a few terms in use in standard works of original research or of reference, published within the last thirty-two years, which presented themselves the first to our hand; viz.—"The Miscellaneous Botanical Works of Robert Brown" (1866); Mr. Currey's translation of "Hofmeister on the Higher Cryptogamia, &c." (1872); Berkeley's "Introduction to Cryptogamic Botany" (1857); and Bentley's "Manual of Botany" (2nd ed., 1870):—

Achænium	Bentley
Anthera	Brown
Arillus	Bentley
Bractea	Brown
Carpellum	Brown
Integumentum	Berkeley
Involucrum	Brown
Ovarium	Brown
Ovulum	Brown
Perianthium	Brown
Pericarpium	Brown
Pistillum	Brown
Rhizoma	Berkeley
Spermatozoon	Currey
Stamina (plural)	Brown
Stipula	Currey

With the exception of words which have been incorporated into our language, such as *corolla*, *nucleus*, &c., comparatively few of those used in describing flowering plants now retain their classical forms; the most conspicuous exceptions being those applied to the structure of tissues, such as *epidermis* and those ending in *enchyma;* and can anything be more puzzling than the forms in common use for the terms derived from the Greek δέρμα—*epidermis*, *hypoderma*, and *periderm?* We have no doubt that, had our critic lived in the days of Robert Brown and Lindley, he would have thought all the innovations introduced by the latter "uncouth" simply because we were not used to them; and would have said that Lindley had "gone much too far." In some of those adopted by ourselves we have, in fact, been forestalled by others, as in the cases of *antherid* and *archegone* by Lindley, and *sporange* by Oliver.

We now come to the charge made by our critic in the *Academy*, that the terms we have introduced would "puzzle foreigners." Unfortunately, our polyglottism, or rather oligoglottism, will not allow us to vie with our reviewer in his acquaintance with every European language; we are compelled to confine ourselves chiefly to three; but these include by far the greater part of European botanical literature—in fact, every treatise which nine out of ten English readers will wish to consult in the original. The statement quoted above seems to have been rashly made.

In *Italian*, as far as our knowledge goes, the practice is absolutely uniform: no botanical writer of repute uses the classical forms; but every technical term has its Italian spelling and termination. To such an extent is this adaptation to the laws of orthography of the language carried, that we find "xylem" converted into *xilema*, "phloem" into *floema*, "hormogonium" into *ormogonio*, and "hyphæ" into *ife;* and this by the first writers "on special subjects."

Our acquaintance with *Swedish*, *Danish*, *Dutch*, and *Spanish* is too slight to allow us to speak with confidence; but in all these the general practice is, we believe, the same as in Italian, though not to the same extent; with the best writers, when writing in their own language, the use of terms with Latin or Greek terminations appears to be the exception rather than the rule.

In *French*, the practice is by no means so uniform as in Italian; but still that of the highest authorities is, on the whole, very decidedly in favour of French, rather than Latin or Greek, forms of the words in most common use. From works picked up almost at random, we select the following:—

Anthéridie	Van Tieghem, Guignard, Philibert, De Wildeman, Bornet, Thuret.
Archégone	Van Tieghem.
Baside	Tulasne, Roumeguère (*basidie*, Fayod).
Capitule	Bornet.
Conidie	Costantin, Roumeguère (*conid*, Bornet).
Épiderme	Van Tieghem, Rénault.
Favelle	Bornet, Thuret.
Gamétange	De Wildeman.
Glomérule	Bornet, Tulasne.
Gonidie	De Wildeman.
Hormogonie	Bornet.
Hyphe	De Wildeman.
Nucléole	Guignard.
Oogone	De Wildeman (*oogonie*, Roumeguère).
Opercule	Philibert.
Ostiole	Thuret, De Wildeman.
Paraphyse	De Wildeman.
Parenchyme	Guignard, Heckel, Fayod, Bornet, Tulasne.
Perithéce	Costantin.
Pollinide (Florideæ)	Guignard.
Procarpe	Bornet, Thuret.
Propagule	Bornet.
Prothalle	Guignard.
Pycnide	Costantin, Roumeguère.
Sclérote	Van Tieghem, Fayod.
Sore	Thuret.
Sporange	Bornet, Thuret, Roumeguère, Tulasne, Van Tieghem, De Wildeman, Guignard, Philibert.
Stipe	Fayod, Roumeguère.
Stomate	Philibert, Thuret.
Thalle	Thuret, Gay, De Wildeman, Fayod.
Zoosporange	Flahault.

The great stronghold of the conservatives in terminology is the *German* language. No doubt a large mumber of the best writers do here maintain the classical form of most technical cryptogamic terms, including some in which it has already been abandoned with us, such as *conceptaculum*, *receptaculum*, *stolo*, and *perianthium*, just as we still meet with *ovarium*, *ovulum*, and *protoplasma*. This is no doubt largely due to the greater difficulty which the German language has than the French or our own in naturalizing aliens. But even here the practice is by no means uniform, and Germanized forms are coming yearly more and more into use. In order that there may be no question as to the recency and authority of the examples quoted, the following list has been compiled exclusively from the standard treatises in Schenk's "Handbuch der Botanik"; had other works of equal authority been consulted, the list might have been considerably extended:—

Apophyse	Goebel
Archespor	Goebel
Basidie	Zopf
Carpogon	Falkenberg
Cilie	Zopf
Collenchym	Haberlandt, Zimmermann
Conidie	Zopf
Endospor	Goebel
Enzyme	Zopf
Epithel	Haberlandt
Exospor	Goebel
Hormogon	Zopf
Mycel	Zimmermann
Paraphyse	Zopf
Parenchym	Haberlandt, Zimmermann, Detmer, Schenk, Zopf
Plasmod	Zopf
Prokarp	Falkenberg
Sklerenchym	Haberlandt, Detmer, Schenk
Sporogon	Goebel

We do not mean that these words are exclusively used by the writers quoted; it is not uncommon to find the Latin and the German form used indifferently on the same page. It is noteworthy also that even the most rigid conservatives do not use the Latin form in the plural of such words as "oogonium," "sporangium," "antheridium," "sclerotium," &c., but always the German forms, *Oogonien*, *Sporangien*, *Antheridien*, *Sklerotien*, &c.; such words as "oogonia," "sporangia," "antheridia," "sclerotia," &c., are, as far as our experience goes, to be found only in English and American writings and in Latin diagnoses.

Analyzing, therefore, the statement that the Latin and Greek forms of words used in cryptogamic terminology are "familiar in every treatise hitherto written on the special subject in any European language," we find that in Italian the practice is unanimously, and in French (as also, we believe, in most other European languages) preponderatingly in the opposite direction; and that German is the only widely read language of Continental Europe in which even the weight of authority is still on that side.

There are some terms in which, no doubt, the classical form must be retained, especially those which, when deprived of their classical termination, become monosyllabic, such as "thallus," "sorus," "hypha," and "ascus," just as we still speak of a "corolla," a "stigma," a "hilum," and a "raphe." But, with regard to the great majority of terms in current use in descriptive cryptogamic botany, we entertain not the smallest doubt that the change will gradually be brought about which has, within the last forty years, become established in phanerogamic botany; and we would venture to suggest to our fellow-workers in cryptogamic botany in this country and in America, whether it will not be best to accept it frankly once for all.

ALFRED W. BENNETT.

Exact Thermometry.

I AM quite in agreement with Prof. Sydney Young (NATURE, December 19, p. 152), that after the lapse of a sufficient time—let us say, an infinite time—the constant slow rise of the zero-point of a thermometer at the ordinary temperature will attain a definite limit; but I cannot accept his view that the effect of heating the thermometer to a high temperature is simply to increase the rate at which *this* final state is approached. If the results of experiment at the ordinary temperature be expressed in a mathematical formula which admits of making the time infinite, the limiting value of the rise (on that condition) will not exceed on the average 2° C., even in a thermometer of lead glass. After exposure to a high temperature, and in the same thermometer, so great an ascent as 18° C. is a possible measurement, actually realized. The two phenomena are therefore very different in their nature.

The view that, owing to the more rapid cooling of the outer parts of the bulb after it has been blown, the inner parts are in a state of tension, and that it is the gradual equalization of the tension throughout the glass that causes the contraction, has frequently been held, and will probably be for a long time the favourite hypothesis upon the subject. It breaks down, however, when we attempt to calculate what the amount of the contraction might be, on the supposition that it is well founded: only a very small portion of the contraction could be thus accounted for. I regret that I cannot now conveniently refer to Guillaume's interesting demonstration of this result.

Prof. Young has placed on record an experiment with three thermometers, which he heated to 280° C. The zero movement, however, only ranged from 1° to 1°·2,—small readings which might very possibly have been obtained, or not, on either of the thermometers at other times. It is consequently very difficult to draw any inference from this experiment. I may, however, mention that closed thermometers made of lead glass are very apt to show a rise of zero after heating to about 120° C. and upwards to some temperature in the neighbourhood of 270° C., and after that a descent of zero; the temperature of 280° C. would in that case be an unsatisfactory one for a test experiment, and the effect of plasticity might very possibly be masked. On the other hand, if the three thermometers were of hard glass, all the zero movements would in that case be greatly diminished, and the results would be in less bold relief.

I do not know any substance more curious or interesting in its properties than glass; and I should be glad if Prof. Young—into whose able hands the matter has fallen—could decisively test my suggestion that plasticity is the main cause of the zero ascent after 120° C. Probably it has little or nothing to do with the ascent at the ordinary temperature. It is, however, known that fine threads of glass are undoubtedly plastic at the ordinary temperature.

EDMUND J. MILLS.

Melrose, N.B., December 29, 1889.

THE PALÆONTOLOGICAL EVIDENCE FOR THE TRANSMISSION OF ACQUIRED CHARACTERS.[1]

MUCH of the evidence brought forward in France and Germany in support of the transmission of acquired characters, which has been so ably criticized in Weismann's recent essays, is of a very different order from that forming the main position of the so-called Neo-Lamarckians in America. It is true that most American zoologists, somewhat upon Semper's lines, have supported the theory of the direct action of environment, always assuming, however, the question of transmission. But Cope, the able if somewhat extreme advocate of these views, with Hyatt, Ryder, Brooks, Dall, and others, holding that the survival of the fittest is now amply demonstrated, submit that, in our present need of an explanation of the origin of the fittest, the principle of selection is inadequate, and have brought forward and discussed the evidence for the inherited modifications produced by reactions in the organism itself—in other words, the indirect action of environment. The supposed arguments from pathology and mutilations have not been considered at all: these would involve the immediate inheritance of characters impressed upon the organism and not springing from internal reactions, and thus differ both in the element of time and in their essential principle from the above. As the selection principle is allowed all that Darwin claimed for it in his later writings, this school stands for Lamarckism *plus*—not *versus*—Darwinism, as Lankester has recently put it. There is naturally a diversity of opinion as to how far each of these principles is operative, not that they conflict.

The following views are adopted from those held by Cope and others, so far as they conform to my own observations and apply to the class of variations which come within the range of palæontological evidence. In the life of the individual, adaptation is increased by local and general metatrophic changes, of necessity correlated, which take place most rapidly in the regions of least perfect adaptation, since here the reactions are greatest; the main trend of variation is determined by the slow transmission, not of the full increase of adaptation, but of the disposition to adaptive atrophy or hypertrophy at certain points; the variations thus transmitted are accumulated by the selection of the individuals in which they are most marked and by the extinction of inadaptive varieties or species: selection is thus of the *ensemble* of new and modified characters. Finally, there is sufficient palæontological and morphological evidence that acquired characters, in the above limited sense, are transmitted.

In the present state of discussion, everything turns upon the last proposition. While we freely admit that transmission has been generally assumed, a mass of direct evidence for this assumption has nevertheless been accumulating, chiefly in the field of palæontology. This has evidently not reached Prof. Weismann, for no one could show a fairer controversial spirit, when he states repeatedly: "Not a single fact hitherto brought forward can be accepted as proof of the assumption." It is, of course, possible for a number of writers to fall together into a false line of reasoning from certain facts; it must, however, be pointed out that we are now deciding between two alternatives only, viz. pure selection, and selection *plus* transmission.

The distinctive feature of our rich palæontological evidence is that it covers the entire pedigree of variations: we are present not only at but before birth, so to speak. Among many examples, I shall select here only a single illustration from the mammalian series—the evolution of the molar teeth associated with the peculiar evolution of the feet in the horses. The feet, starting with plantigrade bear-like forms, present a continous series of readjustments of the twenty-six original elements to digitigradism which furnish proof sufficient to the Lamarckian. But, as selectionists would explain this complex development and reduction by panmixia and the selection of favourable fortuitous correlations of elements already present, the teeth render us more direct service in this discussion, since they furnish not only the most intricate correlations and readjustments, but the complete history of the addition

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[1] This article is an informal reply to the position taken by Prof. Weismann in his essays upon heredity. I have borrowed freely from the materials of Cope, Ryder, and others, without thinking it necessary to give acknowledgment in each case.